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THE COLLECTED PAPERS OF PROFESSOR ADAMS.

The Scientific Papers of John Couch Adams, M.A., Sc.D., D.C.L., LL.D., F.R.S., late Lowndean Professor of Astronomy and Geometry in the University of Cambridge. Vol. i. Edited by W. G. Adams, Sc.D., F.R.S. With a memoir by J. W. L. Glaisher, Sc.D., F.R.S. Pp. liv + 502. (Cambridge: University Press, 1896.)

TO collect with completeness, and to edit with care and reverence, the scientific work of those whom the world has followed with close and respectful attention, must be a grateful and, at times, a necessary task. The opportunities of publication are fortunately numerous, for many learned societies and scientific bodies are eager to make known the researches of those whom they are proud to reckon among their associates or supporters. Obviously, this general scattering is attended with many evils, which collection will effectually remove. Not only will readier access be given to the student to those papers in which he is interested, but the reputation of the author will probably be enhanced by the greater coherency which his work is likely to acquire. In the present instance, however, though the result will be welcomed by many, the necessity for collection is not so apparent as in some other famous cases. The number of published papers is not large—sixty-two in all, of which five are addresses from the presidential chair to the recipients of the medals of the Royal Astronomical Society, and a few others, the results of observations connected with the direction of the Cambridge Observatory. Of the more important papers, each is usually complete in itself, since it was seldom necessary for Adams to return to a subject which he had once discussed. Neither did Prof. Adams select many channels by which to make his thoughts and results known to the world. The Astronomical Society sufficed him for the greater part, and their publications are readily accessible, though it must be admitted that the Council of that Society have very recently found themselves compelled to republish one of his more interesting papers.

Dr. Glaisher contributes an appreciative account of Adams's life. Without containing many new facts, it shows a keen insight into his character, and makes the reader understand in some measure, the reason why Adams maintained so pre-eminent a position among his contemporaries. The thoroughness with which he did his work, the innate craving after perfection, the steady maintenance of a high standard of excellence, the dislike to hurried and incomplete publication, all contributed to give him that superiority which he never courted, and to make him an authority whom all respected. Naturally, the history of the discovery of Neptune figures largely in the biographical notice. It is a subject that apparently never wearies, and concerning which the last word will never be written. But all the material facts have long since been threshed out, and there is no new source from which any fresh information can be derived. Dr. Glaisher permits himself to criticise in some particulars the conduct of both Airy and Challis.

"Adams was not fortunate," he says, "in the two astronomers to whom he communicated his results; neither of them gave to a young and retiring man the kind of help or advice that he should have received. . . . As Professor in the University, he (Challis) should not have allowed a young Senior Wrangler, through modesty or diffidence, to do such injustice to himself. . . . It is impossible not to contrast the admiration with which he (Airy) received Le Verrier's published writings with the indifference shown towards Adams's still unpublished work. Adams was certainly as clearly convinced of the reality of the planet as Le Verrier, and whatever claims the latter has to the name of philosopher rather than mathematician, apply equally to the former."

This is no doubt true, but it does not seem to be as distinctly remembered as it should be that Adams in fame and reputation suffered no irreparable injury. Greater attention could not have been drawn to the excellence and originality of his work, or the philosophic character of the original conception, than was occasioned by the untoward circumstances that surround the discovery of Neptune. Though no one sought less for public applause than did Adams, the effect of the controversy was to advertise his merits far more widely than would have been the case, if Le Verrier's papers had not appeared. The immediate effect was to place Adams at one bound in the front rank of astronomers; and perhaps there is no other instance, in scientific life, of a reputation so assured being so swiftly won. He presents the singular—perhaps the unique—instance of a young man being unable to add to his rapidly acquired reputation. That the high level at which he commenced his career was amply sustained, will of course be universally admitted; but it was almost impossible that it could be raised, in the public mind, at least.

The investigation of the perturbations of Uranus, of course, occupies the first place in the collected series of papers; and to this discussion is added, as a matter of historical interest, some of the earliest observations of Neptune made by Prof. Challis, as well as an account of the zonal measurements made at the instance of the late Astronomer Royal, with a view to the detection of the planet prior to the actual discovery at Berlin. More interest, however, attaches to a reference to a reprint of Adams's original paper in Liouville's *Journal de Mathématiques* for 1875, because it is accompanied by a less known note, that Prof. Adams contributed in reply to some criticisms of Prof. Pierce. Very shortly after the discovery of the planet, Prof. Pierce had contended that the true period of Neptune differed so widely from that assigned by Adams to the hypothetical planet, that the discovered object was not the planet indicated by geometry, and that in fact its discovery must be regarded as a happy accident. The main contention of Prof. Pierce was that the commensurability of the mean motion of Uranus with that of Neptune would give rise to perturbations always in the same part of the orbit of the inferior planet, whereas the perturbations due to the hypothetical planet would not occur with the same regularity, but would vary 80° upon the orbit of Uranus, at each successive conjunction and opposition. Adams had little difficulty in disposing of this criticism, though this collection of papers would indicate that he took no notice of it for thirty years. He then points out that the objection would be valid if it were sought to explain the perturbations of Uranus throughout one or more whole synodic periods of the planet. Practically, and for the purposes of the particular solution, the action of the disturbing planet on the motion of Uranus is limited to some twenty years before and after 1822, when the two planets were last in conjunction. The preceding conjunction took place in 1650, some forty years before Flamsteed's earliest observation, and the sensibly elliptic orbit in which Uranus was moving from that time to the beginning of the century, was the elliptic orbit on which the perturbations of Neptune at the last conjunction had been impressed. The outstanding discrepancy exhibited by Flamsteed's observation, in 1690, from the place indicated by the theories of both Adams and Le Verrier, arose from the inadequacy of those theories to represent the place of the planet in the remote past, owing to the erroneous distance assigned to the hypothetical planet.

Passing over a few short papers generally referring to the orbits of comets and of double stars, we come to Adams's work on the Moon. The papers on this subject refer to lunar parallax, the secular acceleration of the moon's mean motion, and critical notices on the lunar theory, both theoretical and numerical. The work on lunar parallax includes an entire revision of the equations from which the parallax is deduced in the tables of Burckhardt and Damoiseau, as well as in the theories of Plana and Pontécoulant. Henderson had found a difference of 1″·3 in the value of the mean parallax deduced from observation, according as he employed the tables of Damoiseau or Burckhardt. Clausen had also called attention to discrepancies in the equations of parallax between the same two authorities, but had not pursued the subject, probably on account of the labour involved. But Adams not only instituted a rigorous comparison between the coefficients employed, and traced the errors of Burckhardt to their source, but added a table of corrections to the daily values of the parallax given in the Nautical Almanac from 1840–1855. This heavy piece of work, demanding as much nicety in its mathematical investigation as patience in its numerical application, Dr. Glaisher describes as characteristic of the author. No part of the work is given ; the method of procedure is briefly sketched, and the final conclusions are stated in the fewest words, and simplest manner possible.

The discussion of the secular acceleration of the moon's mean motion is known almost as well as the history of the discovery of Neptune. Fierce controversy has centred round this question, and forced it on public notice. It is difficult to understand now, and impossible to explain briefly, the reason for the controversy called forth by Adams's paper in 1853. This somewhat acrimonious debate was carried on with undiminished force for some years, though Adams took little part in it beyond practically settling the fray in 1860. To Adams, and to mathematicians of the present day, the problem is purely one of dynamics. Given that the eccentricity of the earth's orbit changes at a slow uniform rate, to determine the corresponding rate of change in the mean motion of the moon. The solution of this problem is, as Adams pointed out, to be effected by means of a purely algebraical process, the validity of each step of which, admits of being placed beyond all possible doubt. Here there would seem to be no room for dispute. But the question did not present itself quite in this simple manner to Laplace and others of his time. They always had before them the necessity of explaining on purely gravitational grounds the observed motion of the moon. The complete vindication of the Newtonian theory was dear to the hearts of the schoolmen of the last century. Many triumphs had been successively won by investigations undertaken with this object, the greatest of which was due to Laplace, who was believed to have satisfactorily explained and laid to rest the last difficulty, revealed by the complicated motion of the moon. In his paper of 1853, Adams joined issue with Laplace, and in showing that the work of the earlier astronomer was incomplete, he not only destroyed the harmony that was so long supposed to exist between observation and theory, but practically impeached the judgment of those who upheld the authority of Laplace. M. de Pontécoulant seems to have urged, as a reason for the non-acceptance of Adams's value, the fact that it had "l'inconvénient d'altérer profondément l'expression analytique admise jusqu'à present, au coefficient de cette equation."

It is curious to reflect on what slender grounds the observed value of 10″ assigned to the secular acceleration was based, and, consequently, how little increased weight it could add to any theoretical value with which it chanced to accord. We may take Dunthorne's investigation as typical of the others made with the same purpose. He computed from the lunar tables in use in his time, probably those published in 1739, and in which the moon's centennial mean motion would have a considerable error, the circumstances of the eclipses recorded by Tycho Brahe, by Regiomontanus (1478–1490), by Ebn Jounis (977–8), the eclipse of Theon (364), and those of Ptolemy. Not one of these sources is unobjectionable, or possesses the necessary accuracy. Ptolemy's catalogue of eclipses was probably selected by him to satisfy a preconceived theory. Theon says that he computed the time of the beginning of the eclipse, and found the observation agreed with his calculation. This agreement is suspicious. The Arabian observations, eye determinations of the beginning and end of the phenomena, are the best suited to the purpose ; though here the error in the moon's mean longitude must be considerable, and a secular acceleration of 7″ will satisfy the observations. Those of Regiomontanus and Tycho Brahe, still made without a telescope, must have approximately the same error, and are too near in point of time to gain the advantage of accumulation.

It is scarcely necessary now to point out the source of the error in Laplace's theory. In this error he was followed by Damoiseau and Plana, who, while extending the method to include the square and higher powers of the disturbing force, failed to detect the incompleteness of the reasoning which vitiated the earlier portion of the work. Laplace only took into account directly the radial component of the disturbing action of the sun.

This neglect of the tangential disturbing force, or the assumption that the area described in a given time by the moon about the earth undergoes no permanent alteration, introduced a considerable error in the coefficient of m^4 in the expression for the true longitude. Correcting the whole of the coefficients in the expansion as far as m^7, Adams assigned a value to the secular acceleration that has not been sensibly disturbed by any subsequent investigation. This value is 5″·7, a quantity only about one-half of that assigned by Laplace, or of that which seemed to be demanded by observation. The minuteness of the quantity sought affords a good illustration of the powers of analysis. The acceleration of the moon's motion implies, of course, an approach to the earth, but the amount is less than one inch per annum, and this minute quantity is determined by theory to within about one-thousandth part of its true amount.

Such an investigation as this last, exhibits the patient examination which Adams was prepared to give to a term, the value of which has been frequently under review by methods that have long been pursued. It shows a confident reliance upon the accuracy of his judgment, the completeness of his work, and a refusal to be led by authority. But some later papers on the general method of treating the lunar theory will be better appreciated, as showing, perhaps, greater originality, and illustrating the application of the results of more modern mathematical inquiry to obtain greater accuracy with less expenditure of labour. It is curious to notice that but for a lucky accident, this peculiar method of treating the lunar motion, and which is likely to be much developed in the future, would not have been published by Adams himself. In 1877, Mr. Hill published his inquiry into the motion of the moon's perigee, in which is sought an absolutely accurate value of that part of c (the ratio of the synodic to the anomalistic months) which depends upon m alone. This is the historic problem that Clairaut successfully solved by adding the term depending upon m^3, and thus supplying a confirmation of the Newtonian theory when it was most needed. Delaunay has since determined the numerical value of the series as far as m^9, and possibly human patience could get little further by this process. Mr. Hill had recourse to quite a different method, which, as applied, gives the same accuracy that would be attained by carrying the series to m^{27}. The sight of this paper by Mr. Hill, seems to have reminded Prof. Adams that some ten years previously he had been at work on similar lines in order to arrive at an accurate value of g, which is related to the motion of the node in the same way that c is to the perigee. The differential equation which determines z, the moon's coordinate perpendicular to the ecliptic, is

$$\frac{d^2z}{dt^2} + \left(\frac{\mu}{r^3} + \frac{\mu'}{r_1^{\,3}}\right)z = 0.$$

Prof. Adams puts

$$\frac{\mu}{r^3} + \frac{\mu'}{r_1^{\,3}} = (n - n')^2\{q^2 + 2q_1 \cos 2(n - n')t + 2q_2 \cos 4(n - n')t + 2q_3 \cos 6(n - n')t + \&c.\},$$

and on solving this equation was led to the form that Mr. Hill had employed in his work. For Mr. Hill had made the general equations of motion depend upon a single differential equation having the form

$$\frac{d^2w}{d\tau^2} + \Theta w = 0,$$

where τ denotes the mean angular distance between the sun and moon, and Θ can be developed in a periodic series of the form

$$\Theta_0 + \Theta_1 \cos 2\tau + \Theta_2 \cos 4\tau + \&c.,$$

leading to the same infinite determinant in both cases. This is developed in a series of powers and products of small quantities, the coefficient of each term being given in a finite form. The similarity of method pursued independently by the two mathematicians, and the greater accuracy obtainable with less labour, seem to point to a new departure in the method of treating the lunar theory. Prof. Adams has indicated what appears to him the most advantageous method of treating this problem.

We can but barely mention one other of Adams's investigations, the discussion of the orbit of the November meteors. It is well known that the late Prof. Newton left undecided the periodic time in which the meteors revolved about the sun, indicating, however, the method which might lead to the settlement of the question by the discussion of the observed amount of secular perturbation of the node. By a method given by Gauss in his "Determinatio Attractionis," it is shown how to determine the attraction of an elliptic ring, such as the meteors form, on a point in any given position. By dividing the orbit of the meteors into a number of small portions, and summing up the changes corresponding to these portions, Adams found the total secular changes of the elements produced in each of the five possible periods that Prof. Newton showed might be assigned as the meteoric path. With only one of these periods, that of about thirty-three years, was it possible for the node to advance in the manner required by the several historical accounts of the meteoric display. With a thirty-three years' period, and with no other, the longitude of the node is increased 20′ by the action of Jupiter, 7′ by that of Saturn, and 1′ by Uranus, thus 28′ in all, giving a mean annual motion of 52″, agreeing with the observed motion, and thus satisfactorily settling the periodic time in which the November meteors revolve.

Several other papers possess great interest, and evidence among other things much painstaking arithmetic. Such is the calculation of thirty-one of Bernoulli's numbers, and the computation of the Eulerian constant to 263 places of decimals. These may have been the occupation of his leisure moments. The reputation of Adams will ever rest upon the determination of the inverse perturbations of Uranus, the work on the lunar theory, and his inquiry into the period of the November meteors.

A CYCLOPÆDIA OF BIOLOGICAL THEORY.

Les Théories sur l'Hérédité et les grands problèmes de la Biologie générale. Par Yves Delages, Professeur à la Sorbonne. Pp. xiv + 878. (Paris: Reinwald, 1895.)

PROFESSOR DELAGES has produced in this large volume of 880 pp. royal octavo, a valuable exposition and critical discussion of the modern theories bearing upon the great problems of biology. The work is remarkable for the ability with which so vast a variety of theories and observations are epitomised and considered. Whilst the author does not profess to give